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Military Innovation Studies: multidisciplinary or lacking discipline?

Introduction

Military innovation is an important field of academic enquiry which has the potential to make a major pure and applied contribution to research across a number of disciplines. At present, the influence of military innovation scholarship is minimal outside of a very specific scholar-practitioner community working within the field itself. This relatively niche status within security studies and, to a lesser extent, military history is discordant with the field's engagement with, and relevance to, other disciplines. Core aspects of military innovation studies' research agenda such as the dynamics of organisational culture, the challenges of institutional learning and the relative influence of internal and external factors upon change have traditionally been acknowledged as important themes within other social science disciplines such as management, sociology, education, anthropology and psychology yet research into the drivers of military innovation remains, at best, peripheral to each discipline. Further, many of the theoretical constructs now used to explain how such factors influence military innovation were imported from those disciplines so it seems puzzling that the resultant research has not had a much wider multidisciplinary impact. This article demonstrates why military innovation is important to a broader research community within the humanities and social sciences whilst answering why this has not been the case to date. It analyses the recent evolution of military innovation theory, posing important questions about its current state of health and discussing its future direction. While the article commends the increasingly rich and diverse literature in the field it also sounds a warning for the consequences of rushing seemingly applicable theory into the corpus before going on to recommend avenues for further research. Three issues are explored in depth: philosophical tensions between the main approaches to military innovation, the challenges of a multidisciplinary approach and the utility of viewing military innovation studies as a field in its own right. It will conclude by looking to the future; suggesting ways of rising to the challenges faced and increasing its multidisciplinary impact.

There are several reasons why it is important to address these issues now. It is a decade since Adam Grissom wrote an influential article, *'The future of military innovation studies'*¹. In it, Grissom surveyed the field, categorised its scholarship and highlighted gaps in the research agenda. Since Grissom's article there has been an explosion of new and exciting research, partly in answer to his call for more research into bottom-up adaptation but mainly in response to the challenges of learning appropriate lessons from the campaigns in Iraq and Afghanistan. Grissom caught the zeitgeist as many scholars and practitioners had already begun to respond to the issues exposed by conducting cutting edge research on bottom-up and horizontal innovation, the significance of strategic culture and the peculiarities of institutional learning in the context of military organisations. If the themes were not new, the increasingly sophisticated multidisciplinary character of the research certainly was. In consequence, though recent research in military innovation has enriched the field of study enormously by adding a wealth of new empirical studies and drawing upon an array of concepts and theories from multiple disciplines, it has also become extremely intricate and even contradictory. This is not necessarily a bad thing, of course: increased complexity can be a mark of greater sophistication. However, it can also obfuscate and confuse if left unquestioned.

¹ Adam Grissom, 'The future of military innovation studies', *Journal of Strategic Studies* 29/5 (2006), 905-934.

As the remarkable growth of the field over the past decade attests, we have experienced a period particularly conducive to research into military innovation but this is not guaranteed to continue. Much of the cutting-edge research is closely associated with, or even dependent upon, an unprecedented degree of openness from militaries to academic scrutiny. This openness reflects a strong desire on the part of these (predominantly) Western militaries to better understand their own organisational cultures in order to adapt and innovate more effectively. However, as the threat environment transforms it is not a given that practitioner interest in academic scholarship about military innovation will remain and this could have profound consequences for the future of the field. Though the imperative for militaries to learn, adapt and predict remains in both war and peace, there is a natural tendency for military organisations to pay the most attention to the dynamics of innovation when they either perceive themselves ill-prepared for an imminent threat or ill-suited to cope with an existing one: basically, when the character of a given conflict makes them feel most vulnerable. For these reasons it seems an appropriate point to pause, reflect and re-orientate.

Military innovation: a vibrant field of study

There is no question that the past fifteen years has been an immensely productive time for research into military innovation. Overall, the breadth and quality of scholarship has been outstanding and research productivity has only intensified since Grissom's 2006 article. Whether this burst of research energy was inspired by Grissom or simply captured by him is an interesting question. The answer, of course, is a bit of both and this is illuminating because, on the one hand, Grissom identified gaps in the field and encouraged others to fill them but, on the other, Western militaries' increasing willingness to engage with the academic community to understand and learn from the wars they were fighting in Iraq and Afghanistan provided opportunities for unparalleled access to the subjects themselves. The resulting rich vein of research is discussed below.

In his survey of the field, Grissom identified four primary schools of thought on military innovation focussing on four distinct themes: civil-military relations, inter-service politics, intra-service politics and cultural influences.² First, the civil-military relations school is largely associated with the work of Barry R Posen, often cited as the father of the field of military innovation studies. His 1984 work, *The Sources of Military Doctrine*³, posits that the relationship between empowered external agents and creative insiders ('mavericks') defines the character and degree of innovation. Posen's work set the tone for inter-disciplinary research in the field, using as it does three historical case studies of inter-war military doctrine in Britain, France and Germany to test his thesis. Many others, notably Deborah Avant,⁴ have built on this work with compelling empirical studies and it remains at the heart of much military innovation theory today. Second, inter-service politics models, often viewed

² Grissom, 'The future of military innovation studies', 908. For a different approach to surveying the field, see Suzanne C Nielsen, *An Army Transformed: The U.S. Army's Post-Vietnam Recovery and the Dynamics of Change in Military Organizations*, (US Army War College, Carlisle PA : Strategic Studies Institute Letort Paper 2010), 5-33.

³ Barry R. Posen, *The Sources of Military Doctrine: France, Britain, and Germany between the World Wars*, (Ithaca, NY: Cornell UP 1984).

⁴ Deborah D. Avant, *Political Institutions and Military Change: Lessons from Peripheral Wars* (Ithaca, NY: Cornell UP 1994).

as stemming from the work of Andrew Bacevich⁵ though actually not uncommon in other studies of the development of US weapons programmes during the Cold War,⁶ emphasises the way in which inter-service rivalry influences both the type of innovation and perceptions of conflict itself. Third, intra-service politics paradigms contend that competition between branches within a service is the critical driver of innovation. This school of thought is largely linked to the work of Stephen P Rosen's 1991 book *Winning the Next War*⁷ which argued that successful innovation is heavily reliant on what he terms the 'alignment' of leadership, branches and institutional structures. Rosen argued that 'a new theory of victory' emerges when an intra-service ideological struggle is won and wider support garnered. Others developed some of Rosen's themes further⁸ and the concept of innovation being linked to a 'new theory of victory' entered the vocabulary of the field more generally. Finally, the fourth 'school' is perhaps more accurately described as a collection of related approaches privileging cultural factors. Flowing from work on strategic and organisational culture and often drawing upon sociology and anthropology, cultural approaches to understanding innovation have become increasingly prevalent with scholars such as Theo Farrell and Terry Terriff bringing studies of the cultural dynamics that influence innovation into the mainstream literature. Though cultural studies were not unknown⁹ in the field of military innovation, Farrell's substantial body of work in particular both inspired and reflected emerging scholarship that actively pushed the previous boundaries of the field. Farrell and Terriff's collaborative work posited that innovation was inspired in three ways: senior leadership could reshape culture and thus drive innovation, it could be forced by external shock or it could evolve as a result of emulation of other professional militaries.¹⁰ Since then, scholarship that has privileged, or at the very least recognised, the importance of organisational and strategic culture has largely dominated the field.¹¹

⁵ Andrew J. Bacevich, *The Pentomic Era: the US Army between Korea and Vietnam* (Washington DC: National Defense UP 1986).

⁶ See for instance Harvey M. Sapolsky, *Polaris System and Development: Bureaucratic and Programmatic Success in Government* (Cambridge, MA: Harvard UP 1972) and Owen R. Cote, *The Politics of Innovative Military Doctrine: the US Navy and Fleet Ballistic Missiles* (PhD thesis submitted to the Dept of Political Science: Massachusetts Institute of Technology 1996) available at <http://dspace.mit.edu> (DSpace@MIT)

⁷ Stephen P. Rosen, *Winning the Next War: Innovation and the Modern Military* (Ithaca, NY: Cornell University Press 1991).

⁸ See for instance B Hayes and D Smith (eds.), *The Politics of Naval Innovation* (Newport, Rhode Island: US Naval War College 1994); Susan L Marquis, *Unconventional Warfare: Rebuilding US Special Operations Forces* (Washington DC: Brookings Institute 1997); Suzanne C Nielsen, *Preparing for War: The Dynamics of Peacetime Military Reform*, (Cambridge, MA: Harvard University PhD dissertation 2003).

⁹ See for instance, Elizabeth Kier, 'Culture and Military Doctrine: France between the Wars', *International Security* 19/4, (Spring 1995); Elizabeth Kier, *Imagining War: French and British Military Doctrine between the Wars* (Princeton, NJ: Princeton UP 1997); Andrew F Krepinevich, *The Army in Vietnam* (Baltimore MD: Johns Hopkins University Press 1986). More broadly, Graham Allison's seminal *Essence of Decision* was instrumental in demonstrating the importance of organisational culture to national security decision-making, shaping the field of study ever since. Graham Allison, *Essence of Decision: Explaining the Cuban Missile Crisis* (Boston, MA: Little Brown 1971). See also Morton Halperin, *Bureaucratic Politics and Foreign Policy* (Washington DC: Brookings Institute 1974).

¹⁰ Theo G. Farrell and Terry Terriff, *The Sources of Military Change: culture, politics, technology* (Boulder, CO: Lynne Rienner, 2002); Theo Farrell, *Norms of War: Cultural Beliefs and Modern Conflict* (Boulder, CO: Lynne Rienner 2005); Terry Terriff, 'Innovate or Die: Organizational Culture and the Origins of Maneuver Warfare in the U.S. Marine Corps', *Journal of Strategic Studies* 29/3 (2006) 475-503; Theo Farrell, 'The dynamics of British military transformation', *International Affairs* 84/4 (2008) 777-807.

¹¹ See for instance, Dima Adamsky, *The Culture of Military Innovation: The Impact of Cultural Factors on the Revolution in Military Affairs in Russia, the US, and Israel* (Stanford: Stanford University Press 2010); Farrell and

Why has this been the case? First, cultural approaches always had obvious potential and this has been fulfilled by the predominantly high quality of recent scholarship. Cultural studies have proved particularly adept at addressing some conspicuous gaps in our understanding of how militaries change, notably Grissom's call to redress the imbalance between the primarily top-down emphasis of all four schools and bottom-up and horizontal studies¹² and the somewhat surprising paucity of academic scholarship on 'adaptation' in conflict.¹³ The number of studies of bottom-up and horizontal innovation, especially of field-level adaptation, has grown rapidly in the past decade. In terms of contemporary operations, Theo Farrell, again, undertook important studies of British army adaptation in Afghanistan;¹⁴ James A Russell wrote highly influential studies of US Army adaptation in Iraq¹⁵; Foley, Griffin and McCartney wrote a comparative study of British and American 'Transformation in Contact',¹⁶ and a plethora of excellent studies of adaptation in different armed forces emerged.¹⁷ Importantly, works looking at the other side of the hill, so to speak, explored the dynamics of adaptation and innovation from asymmetric or 'hybrid' state and non-state adversaries such as Iran, Hamas or Hezbollah.¹⁸ Conversely, scholars such as Bjerga and Haaland studied innovation outside of conflict, in their case the evolution of joint doctrine in Norway, illustrating how some manifestations of innovation in 'small states' can be unrelated to operational effectiveness whilst still serving important, even strategic, purposes.¹⁹ In sum, though it would be inappropriate to pigeon-hole all of the above scholarship as cultural studies, the importance of cultural factors to the form and function of military innovation is evident in all of it and now infuses much of the best literature in the field.

Recent research has been receptive of constructive criticism, indicating a healthy and energetic discipline. For instance, scholars very quickly began to critique the initial work on bottom-up and horizontal innovation. Anthony King²⁰ and others queried the level of successful adaptation and genuine change in the British army's approach to Afghanistan. Sergio Catignani pointed out flaws in

Terriff, *The Sources of Military Change*. For a range of different approaches by authors in the field see for instance, Dmitry (Dima) Adamsky and Kjell Inge Bjerga (eds.), *Contemporary Military Innovation: between anticipation and adaptation* (London and NY: Routledge 2012); Theo Farrell, Frans Osinga, James A Russell (eds.), *Military Adaptation in Afghanistan* (Stanford: Stanford University Press 2013).

¹² Grissom, 'The future of military innovation studies', 925-30.

¹³ For studies of adaptation as a form of innovation, see for instance Williamson Murray, *Military Adaptation in War: with fear of change*, (Cambridge: Cambridge University Press 2011); Farrell, Osinga, and Russell (eds.), *Military Adaptation on Afghanistan*.

¹⁴ Theo Farrell, 'Improving in War: Military Adaptation and the British in Helmand Province, Afghanistan, 2006-09', *Journal of Strategic Studies* 33/4 (2010) 567-594.

¹⁵ James A Russell, *Innovation, Transformation and War: Counterinsurgency Operations in Anbar and Ninewa Provinces, Iraq, 2005-2007* (Stanford: Stanford University Press 2011)

¹⁶ Robert T. Foley, Stuart Griffin and Helen McCartney, 'Transformation in contact: learning the lessons of modern war', *International Affairs* 87/2 (2011), 253-270.

¹⁷ For a flavour of the breadth of scholarship see the following edited volumes; Risa A Brooks and Elizabeth A Stanley (eds.), *Creating Military Power: The Sources of Military Effectiveness* (Stanford: Stanford University Press 2007); Adamsky and Bjerga (eds.), *Contemporary Military Innovation*; Farrell, Osinga and Russell (eds.), *Military Adaptation in Afghanistan*.

¹⁸ Itai Brun and Carmit Valensi, 'The Revolution in Military Affairs of the 'Other Side'', in Adamsky and Bjerga (eds.), *Contemporary Military Innovation*, 107-129.

¹⁹ Kjell Inge Bjerga and Torunn Laugen Haaland, 'Doctrinal Innovation in Small States', in Adamsky and Bjerga (eds.), *Contemporary Military Innovation*, 83-106.

²⁰ Anthony King, 'Understanding the Helmand Campaign: British Military Operations in Afghanistan', *International Affairs* 86/2 (2010) 311-332; David Betz and Anthony Cormack, 'Hot War, Cold Comfort: a less optimistic take on the British military in Afghanistan', *RUSI Journal* 154/4 (2009) 26-9.

the assumptions of Farrell, et.al that informal networks of battlefield adaptation were necessarily evidence of institutionalised learning: demonstrating that it may be the case that excellent lower-level adaptation actually *reduces* the imperative for embedded institutional learning.²¹ Serena's work on US military adaptation makes a related point²² and Nina Kollars' comparative analysis of gun-truck adaptation in Iraq and Afghanistan demonstrates how effective 'soldier-led' adaptation to specific challenges can produce ad hoc networks capable of dealing effectively with the task at hand without necessarily delivering more permanent bottom-up innovation: efficient but not sufficient, in her words.²³ Raphael Marcus' recent work on Israeli battlefield adaptation in response to its conflict with Hezbollah draws several of these research strands together, examining the IDF's attempts to institutionalise what had previously largely been ad-hoc learning processes through more formal tactical knowledge management mechanisms.²⁴

Second, timing, it would seem, remains everything as the research agendas of cultural approaches have synchronised with those of militaries themselves over the past decade. Though greater academic interest in both strategic and military culture was evident from the mid-1990s, reflecting the 'critical turn' within IR theory more generally, critical approaches only really impacted meaningfully on the field of military innovation studies a decade later when their practical utility became evident to an influential scholar-practitioner community that understandably tends to privilege applied research. As this community began to re-appraise the drivers of successful military transformations in the face of the nightmarishly complex conflicts in Iraq and Afghanistan so the focus turned first outward to apportion blame externally (usually to poor strategy from civilian leaders) but then inwards to the relationships between organisational cultures and strategic choices. The danger of such effective externalisation of blame was always that it would result in a failure to learn appropriate lessons so it was important that the introspection, when it came, was thorough, robust and honest.²⁵ Al Qaeda, Iraq, Afghanistan and the rise of Islamic State (or Daesh) raised all sorts of difficult questions about the ability and willingness of militaries to adapt successfully to twenty-first century threats. Strategic culture literature became increasingly important to the field of military innovation because it looked both up and out from the organisation (i.e. how does the military's culture influence strategic behaviour of, predominantly, the state?) and down and in to the organisation (i.e. how does its strategic culture shape its own choices, how does this reinforce attitude and behaviour and how can one induce meaningful change?).²⁶ Thus, cultural approaches to understanding how and why military institutions innovate were remarkably well-placed to exploit the unprecedented levels of access that many of those militaries subsequently granted and to win the related research funding.

²¹ Sergio Catignani, 'Getting COIN at the Tactical Level in Afghanistan: Reassessing Counter-Insurgency Adaptation in the British Army', *Journal of Strategic Studies*, 35/4 (2012) 513-539.

²² Chad C Serena, *A Revolution in Military Adaptation: The US Army in the Iraq War* (Washington DC: Georgetown University Press 2011).

²³ Nina A Kollars, 'War's Horizon: Soldier-Led Adaptation in Iraq and Vietnam', *Journal of Strategic Studies*, 38/4 (2015) 529-553.

²⁴ Raphael D Marcus, 'Adaptation in the Israel-Hizballah Conflict: the institutionalization of Lesson-Learning in the IDF', *Journal of Strategic Studies*, 38/4 (2015) 500-528

²⁵ Stuart Griffin, 'Iraq, Afghanistan and the future of British military doctrine: from counterinsurgency to Stabilization', *International Affairs* 87/2 (2011) 317-333.

²⁶ For a good overview of thinking about strategic culture, see Jeffrey S Lantis and Darryl Howlett, 'Strategic Culture', in John Baylis, James J Wirtz, Colin S Gray (eds.), *Strategy and the Contemporary World 4th edition* (Oxford: Oxford University Press 2013) 76-95

Finally, if the past decade has been marked by the rise to prominence of cultural approaches, it has been equally notable for the increasingly multidisciplinary character of the field. Though the majority of the scholarship remains rooted in political science, there is also a long interdisciplinary tradition of historians engaging meaningfully with the theory of military innovation, thus providing a much richer, healthier body of knowledge. This tradition has continued and strengthened with historians embracing theoretical advances in the field and combining them with high quality historical research.²⁷ Further, recent advances reflect the willingness of military innovation researchers to draw upon other social sciences, particularly with regard to group dynamics, management theory and organisational learning. Invaluable insights have been drawn from disciplines such as anthropology, sociology, education and management with relevant concepts and theories from each making their way into the literature. Most obviously, cultural studies draw on anthropology, psychology and sociology whilst education and management have been used to develop conceptual toolboxes to better understand the theory and practice of organisational learning. However, though these trends are indicative of a vibrant, growing field, there are a number of significant challenges that need to be met if it is to continue to flourish.

Three inter-related themes have emerged as recurrent problems within the extant military innovation literature. First, proponents of Grissom's four 'schools' seem remarkably conflict-averse; offering insights to complement and enhance each other's analyses rather than setting out their cases for being genuinely competitive explanatory models of military innovation. In many ways this is an admirable trait in the field of innovation studies, bucking the general trend for vicious academic in-fighting over often irrelevant nuance but it has arguably stunted the growth of the field by glossing over more profound epistemological differences between approaches to innovation and thereby skewing the research agendas. Second, the increasingly multi-disciplinary character of the field has expanded its horizons and added greater substance but has exacerbated pre-existing methodological and philosophical tensions between disciplines. Further, the highly selective use and interpretation of terminology, concepts, models and theoretical frameworks from other disciplines has exposed the field's vulnerability to intertextual issues. Finally, the breadth of scholarship on military innovation coupled with the range of multidisciplinary approaches that are now drawn upon raises a much larger question about the continued utility of viewing military innovation as a field in its own right.

Military innovation: a conservative field?

Given its current predominance one would expect the 'Cultural School' to have articulated some powerful arguments for the primacy of strategic and organisational culture not just in shaping innovation but in defining the very terms in which militaries perceive the changing character of conflict itself. Certainly, some of the strongest proponents of the influence of culture demonstrate how culture exerts that influence across each of the components of fighting power: moral, physical and conceptual. Dima Adamsky's comparative study of military innovation in the US, Israel and Russia is an excellent example here.²⁸ Adamsky's ground-breaking work demonstrates how the distinct strategic cultures of each state not only led to different technological pathways but to

²⁷ See for instance, Jonathan Boff, 'Combined Arms during the Hundred Days Campaign, August-November 1918', *War in History* 17/4 (2010) 459-478; Robert T Foley, 'Dumb donkeys or cunning foxes? Learning in the British and German armies during the Great War', *International Affairs* 90/2 (2014) 279-298

²⁸ Adamsky, *The Culture of Military Innovation*.

divergent understandings of the significance of those technological breakthroughs. As Adamsky demonstrates, strategic culture can be so influential as to induce an almost completely inverse relationship between the ability to conceive, develop and deliver technological breakthroughs and the ability to recognise their import. While it is the United States that delivers revolutionary battlespace capabilities based on the much wider revolution in information technology (that it is also leading), it is the Soviet Union, least capable of keeping up, that is first to recognise that these breakthroughs mark a fundamental change in the character of conflict: a revolution in military affairs (RMA). Israel, meanwhile, is the first to apply these capabilities directly but does so in the most narrowly battlefield (or tactical) manner. What Adamsky brilliantly demonstrates is how strategic culture imbued each military with particular characteristics that shaped the development, use and, ultimately, perception of the very nature of the technology. Equally important, it illustrates how each culture has distinct advantages and disadvantages: there is no such thing as the perfect military institution. Trade-offs are embedded in the very fabric of all military organisations because their strategic cultures are determined by choices made in response to constantly evolving complex relationships between the international environment, the state, society and themselves.

However, Adamsky's work is interesting for another reason: one that points to a major problem within the field. Despite demonstrating the profound impact strategic culture can have on strategic choice, Adamsky, like almost everyone else, stops short of ascribing independent causal power to cultural factors. In fact, he goes out of his way to emphasise the compatibility of his study with ones privileging external factors: in theoretical terms, the most traditional positivist explanatory theories.²⁹ This is not a criticism of Adamsky but it is revealing because it is representative of the vast majority of work in the field, including my own. Though cultural approaches obviously accord culture greater status in *shaping* innovation they do not challenge the primacy of conventionally conceived explanations of what *drives* innovation. Williamson Murray's 2011 study of military adaptation in war acknowledges a great deal of commonality between the general drivers of innovation in peace and those of adaptation in war but argues convincingly that the intense pressures of war privileges obvious traditional drivers (operational necessity and technological advances) for adaptation to be successful. Organisational culture is important to Murray but in a supplementary capacity: "the organizational culture of particular military organizations formed during peacetime will determine how effectively they will adapt to the actual conditions they will face in war."³⁰ Farrell's introduction to *Military Adaptation in Afghanistan* (2013) agrees that operational challenges and new technologies are the drivers of adaptation in warfare, according strategic culture shaping status only in his analytical framework.³¹ Even Elizabeth Kier, who states categorically that 'culture has causal autonomy' subsequently qualifies her argument, undermining the case for the centrality of culture and defending the continued utility of structural and functional analyses.³² Implicit adherence to this hierarchy typifies cultural approaches suggesting that even scholarship ostensibly grounded in more radical epistemological positions gravitates towards the middle-ground and declared compatibility

²⁹ Adamsky, *The Culture of Military Innovation*.

³⁰ Murray, *Military Adaptation in War*, 308-309

³¹ Theo Farrell, 'Introduction: Military Adaptation in War', in Farrell, Osinga and Russell (eds.), *Military Adaptation in Afghanistan*, 1-23 (8).

³² Kier, *Imagining War*, 140 and 164.

with traditional theories.³³ This is superficially counter-intuitive given the current prevalence of cultural approaches to understanding the dynamics of military change. However, if one views it as indicative of a theoretical conservatism partially induced by the heavily practical focus of the field it becomes more comprehensible.

Ironically, scholarship bearing many or all of the hallmarks of critical approaches to theory³⁴ has been successfully integrated into the field, and arguably come to dominate it, precisely because it has not fundamentally challenged the utility of other schools. This is remarkable because if any of Grissom's four competing schools should constitute an existential threat to the others, it is the cultural one. Instead, cultural approaches have emphasised their compatibility not only with intra- and inter-service approaches, which already argued that the internal dynamics of military organisations were crucial to innovation, but also with the seminal work of Posen which appears philosophically furthest removed. This accommodation of other perspectives created considerable room for the exploration of cultural drivers without directly contradicting the central hypotheses of the other schools but has inevitably constrained the cultural, or more broadly critical, research agenda. As each of the other schools assumes the existence of an external reality that sets the requirement for innovation, if not the conditions under which it develops or its ultimate form, they are ultimately highly traditional, positivist explanatory models. Cultural approaches have largely accepted, or ignored, this point and that creates a significant tension in the work which stunts growth in the field because it means that strongly positivist, and largely reductionist systemic IR theories have rather neatly assimilated cultural approaches within their broader epistemological framework.

Perversely, the price paid for the current predominance of the cultural research agenda is subordinate paradigmatic status within military innovation studies. Put simply, the vast majority of cultural scholarship accepts that culture lacks independent causal power. Cultural approaches to military innovation therefore mainly conform to theoretically conservative approaches to strategic culture more generally: providing value-added explanations of strategic behaviour that are epiphenomenal, or at best secondary, to pre-existing, firmly positivist IR theories.³⁵ Studies such as Adamsky's push the boundaries further, demonstrating culture's capacity to act as a conceptual vehicle to understand aspects of behaviour that would otherwise defy explanation but still tend not to confer the status of an independent variable with equal or superior explanatory power to traditional theories.³⁶ They therefore remain very firmly positivist and, ultimately, subordinate. Little or no military innovation literature takes more radical critical, constructivist or even entirely post-positivist approaches by ascribing independent, superior or unquantifiable influence to military culture despite the fact that key theorists have robustly defended its capacity to do so in the past.³⁷

³³ For an excellent introduction to the key debates within International Relations theory, see Tim Dunne, Milja Kurki and Steve Smith (eds.), *International Relations Theories: Discipline and Diversity*, 3rd Ed. (Oxford: Oxford University Press 2013).

³⁴ For a comprehensive study of the relevance of critical approaches to security studies, see K M Fierke, *Critical Approaches to International Security* (London: Polity Press 2007). For an insight into the work of one of the most important thinkers, see James Der Derian, *Critical Practices in International Theory: Selected Essays* (London: Routledge 2009).

³⁵ Lantis and Howlett, 'Strategic Culture' 77-78.

³⁶ Lantis and Howlett, 'Strategic Culture' 78.

³⁷ See for instance, Theo Farrell, John Duffield, Richard Price and Michael C Desch, 'Correspondence: 'Isms and Schisms: Culturalism versus Realism in Security Studies', *International Security*, 24/1 (1999) 156-180.

In summary, though the 'critical turn' within the field of military innovation has been integral to the rise to prominence of cultural approaches the ambition of its research agenda has been severely curtailed by its own theoretical conservatism.

Most likely, this gravitation to the theoretical middle-ground is the result of the aforementioned applied character of the field. Most military innovation scholars are inherently practice-focussed because they routinely work, or have forged good working relationships with, the military and other relevant agencies. Almost all of the research has been undertaken by serving or former officers, defence think tanks and academics closely associated with professional military education (PME) or defence policy. This overlapping scholar-practitioner community has both the greatest access to military organisations and a vested interest in understanding the dynamics of change and innovation within those organisations. It is unsurprising, therefore, that its research outputs not only seek to understand the drivers of innovation but to explain their dynamics *and* apply the knowledge gained. As research funding also often focusses on answering practical questions about how to improve institutional learning, the scholarship naturally asks the 'what' and the 'why' questions in order to provide recommendations, or at least insights, about 'how'. There is therefore an inherent structural bias within the field toward applied research that discourages overly theoretical scholarship.³⁸ This is important not only because it exposes a gap in the field's own theoretical literature but because that gap is reflective of much bigger, on-going debates within International Security Studies and its parent discipline of International Relations (IR) about the relative strengths of competing (and still dominant) positivist IR theories, the explanatory power of 'middle ground' constructivism and critical theory and the practical utility of their more radical manifestations as well as the relevance of entirely post-positivist approaches.³⁹ Addressing a theory gap in our own field could therefore make a major contribution to wider disciplinary debates.

Identification of this theory gap is not to accuse innovation scholars of lacking theoretical grounding, nor is it a criticism of the quality of the various theories of innovation that they have advanced, but it is appropriate to acknowledge that there is a natural tendency within the field to privilege practical utility over theoretical purity and that this tendency has a significant constraining effect on the research agenda. Military innovation scholars tend to be of a type in epistemological terms. Despite protestations to the contrary, "they turn out, in the final analysis, to be ambivalent about how much independent explanatory power cultural variables have"⁴⁰ producing some outstanding but philosophically narrow research. This narrowness opens the door to accusations that the research is niche, 'simply policy-work' or academically less rigorous; none of which being true but all hard to defend against. If it is to do so, the field needs fresh blood to keep it intellectually honest, push boundaries much further, eschew the comfort of the theoretical middle-ground and provoke arguments about the fundamentals. In summary, while it is perhaps unfair to label individual pieces of scholarship as 'conservative' when the quality of research has been so high and the field has come so far, it is a justifiable criticism that military innovation studies' applied bent has focussed

³⁸ See Charles King, 'The Decline of International Studies: Why Flying Blind is Dangerous', *Foreign Affairs* (July/Aug 2015). King makes an important point about how government funding in general and DoD Minerva grants in particular have skewed the research agenda

³⁹ For these debates, see for instance Dunne, Kurki and Smith (eds.), *International Relations Theories: Discipline and Diversity*.

⁴⁰ Michael C Desch, 'Culture versus Structure in Post-9/11 Security Studies', *Strategic Insights* IV/10 (Oct 2005) 8.

competition for theoretical predominance on the centre ground. The positive effect of this has been a reduction in conflict over new thinking that encourages a high degree of receptiveness to alternative perspectives. The negative one has been to mask greater philosophical divisions between more radical expressions of these perspectives, thereby limiting the ambition of the research agenda. Such a cautious approach to theorising is understandable in an applied field but is self-defeating because it engenders an overly restrictive relationship between theory and practice.⁴¹ To use a military innovation metaphor, research and development (R&D) founded on such a relationship will always produce a better battleship but never the aircraft carrier that supersedes it, changing the face of battle at sea.

Military innovation studies: a confused field?

If we need to reflect on the compatibility of the IR foundations of the literature we definitely need more rigorous analysis of the multidisciplinary tools now in use. The field's increasingly multidisciplinary character is a tremendous strength but also makes it vulnerable to the misinterpretation and consequent misapplication of nascent conceptual tools derived from the other disciplines. It is of enormous benefit that a broad range of relevant social sciences are being drawn upon but the highly selective use of theoretical concepts or even entire models is problematic. This problem is compounded by two issues. First, though multiple disciplines are being used, the field remains dominated by political scientists and historians. Though some anthropologists have rather controversially worked with the military, it is only really a select group of sociologists who have engaged directly with the study of military innovation. Second, the applied mentality of the field, discussed above, encourages the integration of promising concepts or theoretical frameworks from multiple disciplines into the extant literature comparatively quickly without necessarily much reflection on where they fit or what the implications are for the pre-existing corpus.

The effect of these two issues is highly problematic. Without any significant academic discourse with target disciplines, the influx of concepts and theories cannot be said to be truly multidisciplinary: it is borrowing rather than sharing. Coupled with the impetus to incorporate new ideas quickly, the field's vulnerability to flawed multi-disciplinary research is comparatively high. To use a literary term, there is an intertextual problem because the texts being referenced are not understood in the appropriate disciplinary context. They are therefore misinterpreted and subsequently incorrectly contextualised before being misapplied throughout the field. By way of example, organisational learning theory has sometimes been dismissed, notably by Grissom⁴², as process-driven or focussed when more thorough examination of the literature reveals it is much more creative and dynamic.⁴³ Despite this reluctance to embrace it, tools, concepts and models with their roots in organisational

⁴¹ For the challenges of developing an effective practitioner-researcher relationship, see Argyris and Schön, *Organizational Learning II*, 30-51.

⁴² Grissom, 'The Future of Military Innovation Studies', 926 (footnote 105).

⁴³ See for instance Chris Argyris and Donald A Schön, *Organizational Learning II: Theory, Method, and Practice*, (Reading, MA: Addison-Wesley 1996); Anthony J Dibella, Edwin C Nevis and Janet M Gould, 'Understanding Organizational Learning Capability', *Journal of Management Studies*, 33/3 (1996) 361-379; Barbara Levitt and James G March, 'Organizational Learning', *Annual Review of Sociology* 14 (1988), 319-340; James G March, *The Pursuit of Organizational Intelligence: Decisions and Learning in Organizations*, (Malden, MA: Blackwell 1999); Peter Senge, *The Fifth Discipline: The Art and Practice of the Learning Organization*, (New York: Doubleday 1990).

learning theory have permeated the field to such an extent that its terminology is now quite commonplace. However, without a thorough grounding in the originating theory (largely drawn from management studies) its concepts can easily be misunderstood and misapplied. The concepts of single- and double-loop learning, for instance, feature quite prominently in discussions of how to improve military institutional learning but are often misrepresented as competing learning processes or, worse, examples of good(double-) or bad (single-) learning when in fact they are neither. Organisational learning theory posits single-loop and double-loop learning as complimentary and essential elements of organisational learning; it is the understanding of the organisational culture and where each fits within its learning infrastructure that is important.⁴⁴ Subtly different concepts such as higher and lower-level learning and adaptive and generative learning are sometimes used interchangeably⁴⁵ without a firm grasp of their nuances, creating yet more unnecessary conceptual confusion.

Organisational theory in general has long been referenced in military innovation literature⁴⁶ and the potential for more specific organisational (or institutional) learning theory to be of enormous value to military innovation studies was highlighted by Richard Downie nearly twenty years ago yet it remains underutilised.⁴⁷ John Nagl's comparative study of British and American approaches to counter-insurgency in Malaya and Vietnam⁴⁸ was one of the first to use such theory systematically. More recently, Sergio Catignani⁴⁹ has referenced it in his work on the modern British army and Abdulkader H Sinno has applied it to the warring factions in Afghanistan⁵⁰ whilst Frank Hoffmann has defended the validity of organisational learning theory and argued for the development of a formal organisational learning model.⁵¹ Similarly, historical scholarship on military innovation often previously drew unconsciously on organisational learning theory but an exciting new wave of historical research on the relationships between formal and informal learning in military organisations is increasingly cognisant of the relevant theory.⁵² However, overall, the application of organisational theory to the field of military innovation remains uneven. It is imperative that concepts drawn from organisational theory are defined and used in the appropriate theoretical context across the field if they are to fulfil their obvious potential.

⁴⁴ Chris Argyris, 'Double-loop Learning in Organizations', *Harvard Business Review* (1977); Argyris and Schön, *Organizational Learning II*.

⁴⁵ For higher and lower-level learning, see Adamsky *The Culture of Military Innovation*. For adaptive and generative learning, see Peter M Senge, *The Fifth Discipline*.

⁴⁶ See earlier references regarding the development of thought on organisational culture. Barry R Posen, arguably the founder of the specific field of military innovation studies, devotes much of chapter 2 (34-80) of *The Sources of Military Doctrine* to core concepts of organisational theory.

⁴⁷ Richard Duncan Downie, *Learning from Conflict: the U.S. Military in Vietnam, El Salvador, and the Drug War*, (Westport, CT: Praeger 1998).

⁴⁸ John A. Nagl, *Learning to eat soup with a knife: counterinsurgency lessons from Malaya and Vietnam* (Chicago: University of Chicago Press 2002).

⁴⁹ Sergio Catignani, 'Coping with Knowledge: Organizational Learning in the British Army', *Journal of Strategic Studies*, 37/1 (2013) 30-64.

⁵⁰ A H Sinno, *Organizations at War in Afghanistan and Beyond*, (Ithaca: Cornell University Press 2008).

⁵¹ Francis G Hoffman, *Learning While Under Fire: Military Change in Wartime*, PhD thesis, War Studies Department, King's College London, 2015.

⁵² See for instance Robert T Foley, 'A Case in Horizontal Military Innovation: the German Army, 1916-1918', *Journal of Strategic Studies*, 35/6 (Dec 2012) 799-827; Foley, 'Dumb Donkeys or Cunning Foxes'.

Even the comparatively straightforward interdisciplinary relationship between political scientists and historians has its problems. The danger of misapplied, misunderstood or inappropriate concepts undermining good empirical research should be obvious but then so too should the possibility that sound theory and promising concepts can be undermined when allied to contested history. Elizabeth Kier's work is important because it is one of very few studies to ascribe genuinely independent explanatory power to organisational culture, using the case study of the evolution of French doctrine between the wars to do so,⁵³ but historical analysis of the development of the French, British and German militaries between the wars has long been a rich, and much contested, vein of research.⁵⁴ Similarly, Nagl's ground-breaking comparative study of British and American counterinsurgency in Malaya and Vietnam⁵⁵ drew important conclusions about the role of military culture and the value of organisational learning theory that have informed much of the current literature on both but subsequent historical research has challenged the basic premise that the British military is simply culturally predisposed to perform well in such environments.⁵⁶ Falsification always becomes more difficult in interdisciplinary research. Ironically, this can be exacerbated by the greater range of empirical research: the more case studies, the less selectivity when applying them. Further, as discussed, the cultural concepts drawn from anthropology and sociology are often far removed philosophically from positivist, empiricist IR approaches or historical methodologies. Thus, the danger of instrumentalising history⁵⁷ is magnified by selectivity biases in appropriation of conceptual tools from other disciplines.

None of the above should be interpreted as an attack on the multidisciplinary character of the field. Nothing could be further from the intent. However, the incorporation of theoretical literature from outside our own field of expertise without meaningful interaction with the scholars themselves is not good practice. At present, the dialogue is rather lop-sided with the apprentice doing most of the talking. The danger is that efforts to push the boundaries of the field actually undermine its credibility as we layer theory upon model upon concept all without accurate reference to the original disciplines. If we do, we are building on quicksand. Put another way, where a virtuous circle of multidisciplinary research should exist, there is a very real danger that a vicious one may take hold.

Military innovation studies: a non-existent field?

What's in a name? Certainly in the case of military innovation, the name says a great deal. It gives form and meaning to the research and confers status upon it as part of a specific field of study. However, the above arguments beg a bigger question of whether military innovation is a scholarly field in its own right? There is a powerful argument to suggest that studying how militaries learn,

⁵³ Kier, *Imagining War*.

⁵⁴ For a highly respected account of French doctrine between the wars that differs in important respects from both Kier and Posen, see Robert A Doughty, *The Seeds of Disaster: the Development of French Army Doctrine, 1919-39*, (Mechanicsburg, PA: Stackpole Books 1985).

⁵⁵ Nagl, *Learning to Eat Soup with a Knife*.

⁵⁶ See for instance David French, *The British Way in Counter-insurgency, 1945-1967*, (Oxford: Oxford University Press 2011); Douglas Porch, *Counterinsurgency: Exposing the Myths of the New Way of War*, (Cambridge: Cambridge University Press 2013); Huw Bennett, 'Enmeshed in Insurgency: Britain's Protracted Retreats from Iraq and Afghanistan', *Small Wars and Insurgencies* 25/3 (June 2014) 501-521.

⁵⁷ Huw Davies, 'The Instrumentalisation of History', blog for Defence-in-Depth defenceindepth.co/2014/09/30/the-instrumentalisation-of-history/.

adapt and innovate is simply an important sub-set of research by, say, management studies, anthropologists, educators, psychologists or sociologists exploring organisational learning in particular or group dynamics in general. Studying how and why militaries innovate provides case studies for scholars from each discipline not the other way around. One could argue that the wider literature surrounding organisational learning and innovation applies to militaries in much the same way as it does to businesses, education or any large organisation and that a field that focuses exclusively on military innovation is in danger of simply obfuscating a well-established practice-focussed theoretical literature. By this logic, the 'schools' within the field are really microcosms of research within their own most closely related disciplines not contributors to a single, coherent body of knowledge on military innovation: militaries are the objects not the subject.

There is strength to this logic as there is to the related argument that 'military innovation studies' is a misnomer in itself. Most of what we characterise as the study of 'innovation' is simply the study of learning and most of that learning doesn't result in 'innovation'. The terminology 'innovation', 'adaptation', 'transformation', 'RMA' is vital but over-emphasis upon it can skew the research agenda because it encourages an almost exclusive focus on 'big' moments when major reform is inspired by a new theory of victory, revolutionary technology or by failure in the crucible of war (often all inter-related). Asking why not reform or innovate when the pressure to do so was intense is sometimes done but rarely when the refusal to do so has been demonstrably the correct decision. Perhaps it is even worth re-visiting some of the case studies of failure to innovate in conflict to ask whether they truly were institutional failures in the greater scheme of things. Was the infamous quote by a senior officer about the Vietnam war "I'll be damned if I permit the United States army, its institutions, its doctrine and its traditions to be destroyed just to win this lousy war"⁵⁸ really as stupid as it seems when placed in the context of the Cold War focus on preparing for conventional warfare in Europe?⁵⁹ Everyday routine, training, doctrine development in stable environments, military education and minor adaptation outside of war may seem dull but can be incredibly revealing about the 'normal' learning cultures of militaries because they reveal more fundamental characteristics. If we're only really talking about the dynamics of individual and organisational learning within a military environment then that reinforces the argument about the lack of a separate field. In many ways, the dangers of misapplying concepts drawn from other disciplines are explicitly exacerbated by the notion that military innovation studies is in some way distinct. Placing military innovation in a wider context would overcome many of the above problems and confer genuinely multidisciplinary relevance upon the research.

The counter, that a distinct (but, crucially, not discrete) field of study is required stems from the argument that militaries are unique because of the nature of their work and the fact that they do not have internal competitors. These factors and the potentially catastrophic consequences of failure make militaries peculiarly resistant to change and therefore highly distinctive organisations.⁶⁰ This is a powerful logic for the retention of a distinct intellectual identity but should be caveated.

⁵⁸ Quote widely ascribed to an anonymous senior officer in Saigon. See Brian M Jenkins, *The Unchangeable War*, RM-6278-2-ARPA (Santa Monica: RAND) 1970, p.3.

⁵⁹ For an interesting study of US military culture and the problems Vietnam posed for it, see Robert M Cassidy, 'Prophets or Praetorians? The Utopian Paradox and the Powell Corollary', *Parameters* (Autumn 2003), 130-143.

⁶⁰ This is a common argument throughout the literature. For a concise summary, see Nielsen, *An Army Transformed*, 11-13.

First, all states with advanced bureaucratic apparatus have, by definition, a number of internal competitors for resource but also, more importantly, as our perception of security threats changes so competition for core military roles increases. The argument at its bluntest, that it is only militaries that close with and kill the enemy has always required qualification but now more so than ever. If combatting global radical extremism really is the ‘struggle of our generation’⁶¹ then the battlespace is so broadly defined that it raises all sorts of difficult questions about who is empowered to wield force on behalf of the state and how they do it. The role of western militaries relative to other security agencies is changing, perhaps more radically than ever before. Second, even if militaries remain unique organisations, there is a dichotomy in the innovation scholarship because it acknowledges the relevance of the wider social science literature on organisational theory and draws heavily on examples from public policy, business and education. Assumptions about resistance to change only sets militaries apart if they are unique in this regard but there is plenty of evidence to the contrary. A recurrent theme in public policy and management literature is organisational resistance to change and there are a wealth of studies of why it occurs and how to overcome it.⁶² Further, there are numerous case studies of military innovation that demonstrate not only great aptitude for dealing with change but considerable appetites for it: it’s therefore a myth that all militaries are resistant to change anyway.⁶³ Separation thus adds a sense of importance but also insulates military innovation studies from theoretical and practical developments that would be of benefit. It further exacerbates intertextual issues by failing to engage the wider community.

However, ultimately there remain compelling reasons for maintaining scholarly independence. Regardless of whether militaries are genuinely unique organisations, their importance makes them worthy of study in their own right. Despite rarely undertaking detailed studies of militaries, organisational theorists have a long tradition of citing military organisations as key case studies for understanding organisational dynamics more broadly. Ironically, many even contend that disciplined scholarly interest in organisations actually originates with studies of the military whilst happily avoiding conducting further research into them.⁶⁴ As military organisations are oft cited but rarely studied outside of military sociology and military innovation, it is highly likely that research would fragment further and its relevance would be diminished should the field be subsumed within ostensibly parent disciplines. Here again, one can draw interesting parallels with, and lessons from, broader ‘macro-level’ IR debates as ever-increasing multi-disciplinarity coupled with epistemological and ontological diversity have posed very similar questions about its own future as a discipline.⁶⁵

⁶¹ This exact term has been used repeatedly by representatives of the UK government since the terrorist attacks in Sousse, Tunisia 26/6/2015. PM David Cameron first used it in the House of Commons on 29 June 2015 having previously used it in media interviews and it became a core theme of subsequent speeches.

⁶² See almost all of the organisational theory and management references cited above.

⁶³ For an interesting recent analysis of military responsiveness, see Meir Finkel, *On Flexibility: Recovery from Technological and Doctrinal Surprise on the Battlefield*, (Stanford, CA: Stanford 2011).

⁶⁴ Much of the Management literature on organisations cites the occasional military case study in support of specific arguments or mentions militaries as ‘traditional’ organisations but rarely conducts more detailed analysis. Interestingly, though James G March’s considerable anthology mainly conforms to the above, this highly influential thinker also considered military organizations important enough to devote a monograph to them. James G March and R Weissinger-Baylon (eds.), *Ambiguity and Command: Organizational Perspectives on Military Decision Making*, (Cambridge, MA: Ballinger 1986).

⁶⁵ See Ole Wæver, ‘Still a Discipline after all these Debates?’, in Dunne, Kurki and Smith (eds.), *International Relations Theories: Discipline and Diversity*, 306-328.

Essentially, though it may have intellectual merit, the argument that military innovation is not a separate field and should instead be situated within wider discourses on organisational culture and organisational learning is only genuinely sound if an appetite exists to do so and it does not. Despite inherent contradictions and problematic terminology, the field of military innovation serves a very important purpose and has relevance well beyond its own boundaries.⁶⁶ To date, that relevance has largely been under-appreciated and retaining a distinct identity whilst recognising the limitations that it imposes would be a major step towards rectifying this.

Military innovation studies: an evolving field

In terms of the future research agenda, there are a number of fascinating avenues deserving greater attention as well as some obvious current omissions.

In terms of what is missing from the current scholarship, we still don't have many good empirical studies of bottom-up learning in peace which may reflect a relative lack of bottom-up influence outside of war or a substantial gap in the literature. Further, the paucity of recent research into the relationship between internal and external drivers of change is another notable omission especially given its centrality to the debate about the dynamics of military innovation and learning. It seems incongruous that academic interest in inter-service and civilian-military explanatory models has not enjoyed a renaissance because the present security environment bears striking similarities to ones where these explanations of innovation were so convincing. More generally, research rarely attempts to discern the comparative learning cultures and capabilities of military organisations unless there is a common theme of successful or unsuccessful adaptation to a very specific challenge (usually a conflict with problematic characteristics or a new, game-changing technology). This is probably because such analyses of everyday organisational learning structures and processes are somewhat dry and uninspiring. However, they provide invaluable raw material for sophisticated comparative analyses of military cultures exposing their strengths, weaknesses and biases as learning organisations. As Murray and Watts have noted, intellectual honesty and critical engagement with sound empirical data on the part of militaries in peacetime may have important consequences in wartime⁶⁷ so it is incumbent upon policy-orientated scholars to produce it and enlightened officers to use it. The lack of such studies may also be a reflection of a subconscious research bias inherent within the field: as the name suggests, 'military innovation studies' naturally focusses on the drivers of change and reform when, in reality, many of those drivers are inherent in the basic mechanics and cultures of organisational learning. With this in mind, it is encouraging to see scholarship that draws heavily on organisational learning theory making its way into the mainstream literature but this remains an under-researched area with enormous potential.

To a certain extent the above gaps in the literature can be explained by the current predominance of cultural approaches but they also highlight a subtler, more profound issue. The development of the field over the 30 years since Posen's seminal work may be more usefully seen as having produced several generations of related scholarship rather than competing schools *per se*. As a result, we have

⁶⁶ Again, Prof Steve Smith makes a very similar point in response to those who argue that International Relations is a misnomer because of the diversity of its theories. Steve Smith, 'Introduction' in Dunne, Kurki and Smith (eds.), *International Relations Theories: Discipline and Diversity*, 1-13, 10.

⁶⁷ Barry D Watts and Williamson Murray, 'Military Innovation in Peacetime', in Williamson Murray and Allan R Millett (eds), *Military Innovation in the Interwar Period* (Cambridge: Cambridge University Press 1996), 369-415.

seen little substantive debate between competing approaches because there have rarely been moments of genuine conflict as one challenges and supersedes another. The field is simultaneously richer and poorer for it. The positive effect has been to reinforce the tendency of military innovation scholars to seek common ground with those approaching the topic from different perspectives, and this has made the field remarkably inclusive. Overall, it is a very good thing to avoid the kind of theoretical polarisation so prevalent in the social sciences.⁶⁸ However, a negative consequence of the lack of confrontation has been to downplay or ignore deeper philosophical differences between explanatory models. With regard to the IR-based innovation literature, the development of a more radically critical research agenda would be of huge benefit. As its epistemological foundations are furthest removed from traditional, positivist explanations it would naturally test the boundaries of what is 'useful' in a practical sense and what is compatible in a philosophical one. More specifically, challenging the orthodoxy that cultural approaches "by themselves, do not provide much additional explanatory power beyond existing structural theories"⁶⁹ would be an enormous contribution to the theoretical well-being of the field. Even if the external threat environment remains the primary driver of innovation, cultural approaches argue that strategic culture has a huge influence on the perceived nature of a threat and organisational culture an equally significant impact upon the subsequent character of any resulting reforms. It therefore seems incongruous that culturalists are so hesitant about taking a leap of faith that culture can and does have considerable explanatory, even causal, power. As Charles King recently commented, small 'c' theoretical conservatism is a widespread problem within the broader discipline of international studies but a research environment where "being monumentally wrong is less attractive than being unimportantly right"⁷⁰ is never going to be conducive to genuinely ground-breaking scholarship. A more radical 'critical turn' within military innovation studies could provide the impetus for another burst of exciting new scholarship even if its direct applied value is more limited.

Similarly, with regard to the multidisciplinary literature, we need to test our theories much more robustly, opening them to scrutiny from the disciplines that we are increasingly drawing upon. It is a great thing that sociologists, anthropologists, psychologists and historians are engaging with the field but political scientists have nevertheless largely been on receive, incorporating what we like, discarding what we do not. Intertextual problems between disciplines and reliance on received wisdom are real problems that undermine the credibility of what should be a naturally multidisciplinary field. Perhaps applying critical discourse analysis to the evolution of the corpus would be a rewarding and revealing exercise for some suitably qualified and enthusiastic colleagues? Certainly, if it is to continue to evolve conceptually and practically, it is long past time to expose what is going on to a wider academic audience and prepare to hear things that we may or may not like. In doing so, not only are the foundations of the field strengthened but the quality and relevance of its research is highlighted to the very disciplines that we are most keen to engage. At the very least, this will ensure that the field's research remains credible but it is to be hoped that the effect is much greater, broadening its multidisciplinary horizons and, consequently, its multidisciplinary impact.

⁶⁸ Bridget Somekh, 'Key Features of Research in the Social Sciences', in Bridget Somekh and Cathy Lewin (eds.), *Research Methods in the Social Sciences*, (London: Sage 2005), 2.

⁶⁹ Desch, 'Culture vs Structure', 2.

⁷⁰ Charles King, 'The Decline of International Studies'.

Conclusion

There is no doubt that military innovation is a vibrant field of study and the critiques offered in this article must be taken in that context. Over the past decade, the research agenda has broadened and a wealth of outstanding scholarship has emerged. Even more promising is that a new generation of scholars are not only receptive to a wide range of theoretical approaches to the study of military innovation but are naturally inclined to incorporate them within their work. Academics have benefited from the support of military practitioners who were themselves prompted to take a greater interest in their own organisational learning dynamics by the travails of Iraq and Afghanistan and the uncomfortable notion that they may not be prepared or suited for challenges to come. The wealth of empirical data on the drivers of innovation has expanded enormously as contemporary conflicts have been examined in detail and historical case studies have continued to grow. Further, the more regular application of different theoretical approaches to these studies has added depth and conceptual sophistication to the field which has become increasingly multidisciplinary in the process. In short, the field has changed and advanced over the ten years since Grissom's review.

However, a consequence of these largely positive developments is that military innovation studies is more congested than ever and has a strongly applied focus. In our rush to develop and apply new theories, or to enhance the old ones with wider social science research, there is a very real danger that we have come to a critical juncture without really perceiving it as such. Grissom, in championing corrective action within the field, cited Thomas Kuhn⁷¹ in the narrower sense of addressing the gap in bottom-up studies⁷² but perhaps it is time to reflect more deeply on the soundness of the theoretical foundations of the entire field. Has some of the conceptual terminology that now infuses the literature made its way into our own work without us really querying its validity? Even if we are using the right tools, do we fully understand them? To what extent have we sacrificed more challenging philosophical enquiry for applied utility? Crucially, where do we go next?

It is important to ask these questions now because there is no guarantee that practitioner support will remain. Will the requirement to constantly question and reassess learning capability wane as conflicts that have defined Anglo-American thinking recede into the background? Historically, there has rarely been a strong impetus for militaries to focus on their qualities as learning organisations outside of war unless radical technological breakthroughs or observation of other conflicts provides a shock. Strategic shocks, however, usually only occur with the arrival of war. If interest does remain, it will most likely be in 'innovation' for the sake of 'efficiency', or, in plain English, cost-cutting. This is certainly an emergent theme in the UK where more regular defence reviews and continuing financial pressures are forcing senior MoD officials to encourage 'innovation' from their subordinates.⁷³ Budgetary constraints and austerity measures are, of course, important innovation drivers in themselves but require very different types of studies than the ones that currently dominate the field. Conversely, even if the Paris terrorist attacks of 13 November 2015 does prove to constitute a strategic shock, inspiring longer-term increases in defence spending and provoking greater direct Western intervention in Syria and elsewhere, the blatantly obvious requirement to re-think the

⁷¹ Thomas S Kuhn, *The Structure of Scientific Revolutions*, (Chicago: University of Chicago Press 1962)

⁷² Grissom, 'The Future of Military Innovation Studies', 924.

⁷³ Comments by senior MoD officials (anonymously attributed) at the inaugural King's College London Military Innovation and Learning Research Group roundtable, 17 June 2015, Joint Services Command and Staff College, Defence Academy of the UK.

strategy, tactics and capabilities necessary to confront global terrorism more effectively will almost certainly require some degree of transformation within Western militaries. If military innovation is to continue to flourish as a credible, independent field, we need to be absolutely confident in its ability to answer the practical and conceptual challenges laid out above. It is vital that our academic field is sufficiently robust to ask the right questions in the right context and defend itself from accusations of lack of rigour.

This article has examined the evolution of the field of military innovation studies in order to reflect upon just how far it has come in its relatively short history but, more importantly to offer a constructive critique of its current state in order to articulate a path forward. It has advocated multiple research *pathways* to greater disciplinary maturity. This is a crucial point because though the field can learn much from other related disciplines about how to draw its disparate strands together, it more urgently requires greater diversity and more radical research agendas to propel it forwards. Broadly speaking, there are three things that need to be done. First, we need to re-visit the contending schools of thought about military innovation and engage in a more robust debate about their relative merits as explanatory models. Their common roots in traditional positivist epistemology make comparative analysis relatively straightforward and the range of empirical studies that have already been undertaken gives us a wealth of data to interrogate and re-interpret through each lens. At present, the easy truce between each school is partly the result of their generational development and a consequent inferred mutual compatibility rather than rigorous comparative analysis. Projecting forward, such a debate will either demonstrate genuine mutual compatibility, thus increasing the sophistication of our understanding of the relationship between drivers of innovation, or it will expose deeper incompatibilities and inspire greater competition between them. If one studies the evolution of deterrence theory, one can see striking similarities from which we may learn lessons about how to draw disparate research strands together: the first step being critical engagement with each other's work. As Jeffrey Knopf's review of the evolution of deterrence theory illustrates, it, too, can be usefully categorised as developing in waves, each with very distinct characteristics; it draws on a number of related disciplines, often requiring specific technical knowledge; and it has sometimes under-theorised as a result of its policy-oriented character.⁷⁴ Interestingly, the work of eminent scholars such as Lawrence Freedman has done much to synthesise different realist perspectives but has also inspired greater theoretical diversity by drawing on constructivist ideas as well.⁷⁵ Regardless of whether deeper comparative analysis of the roots of military innovation studies produces a more parsimonious or more diverse theoretical literature, it can only enhance the academic credibility of the field.

Second, we need to encourage cultural scholars to radicalise their research agendas in two opposite directions. The first direction is further towards positivist approaches but with an explicit agenda of challenging the subordinate status of cultural approaches. As discussed, it is already incongruous that the approach that currently dominates the research agenda of the field is simultaneously the one that has generated the least independent traction. A debate between the schools would be enriched enormously by the emergence of more assertive cultural explanations of innovation. The second, opposite, direction for cultural scholarship is to distance itself further from positivist approaches and more fully embrace a new critical turn. This recommendation is not about

⁷⁴ Jeffrey W Knopf, 'The Fourth Wave in Deterrence Research', *Contemporary Security Policy* 31/1 (2010), 1-33.

⁷⁵ See for instance, Lawrence Freedman, *Deterrence* (Cambridge: Polity 2004).

engendering greater coherence within the field: it is about research diversity and the broader IR landscape. Critical analyses of specific national military cultures would undoubtedly deepen our understanding of the unique cultural dynamics of each individual case study and therefore have practical utility to positivist explanatory models. However, 'overextending critical theory in a policy and practical direction... can also pose the challenge of sustaining critical theory's power of critique'⁷⁶ so must be treated with caution. Instead, the primary contribution would be to the wider IR field of critical security studies. How military culture is influenced by strategic culture, and vice versa, how it shapes innovation choices and how those choices, in turn, impact upon the dynamic of international security is a fascinating research avenue with the potential to make a major contribution to critical security studies.

Third, much of this article has been concerned with the ways in which military innovation can better capitalise on its increasingly multidisciplinary character. It is a false assumption that the field is genuinely multidisciplinary at present and this is damaging to the long-term future of several exciting research agendas that draw heavily on other disciplines. At present, most scholarship is interdisciplinary (historical and political science) but often borrows liberally from other related fields without really understanding them. The sometimes excellent use but often ill-conceived abuse of concepts associated with organisational theory is perhaps the most obvious example of this but it is only one example. If nothing else, it is simply good housekeeping to tidy up the field's lexicon and contextualise its theory appropriately. However, the requirement to engage more directly with related disciplines is about much more than just good housekeeping: it is a gold mine of ideas, conceptual tools, theories and empirical data that has a huge amount to offer. This is already evident in current efforts at multidisciplinary research which *do* show genuine promise but presently lack theoretical resilience as a result of a lack of contextualisation within parent disciplines. A concerted effort to engage scholars from other disciplines in our research projects is a prerequisite for the field to evolve.

In summary, sound theory was, is and will remain fundamental to cutting-edge research. The applied bent of military innovation studies is integral to its character but it does increase the temptation to rush promising lines of academic enquiry into the mainstream debate. Very often, military practitioners want specific solutions to practical problems and because their questions broadly align with our own research interests we are all too ready to provide them with answers when we should be more circumspect. In reality, we cannot provide militaries with some kind of silver bullet for creating the perfect learning organisation because there is no such thing but we can produce world-class research from which can be derived conceptual tools and empirical evidence for militaries to better understand themselves and the context within which they operate. Sophisticated multidisciplinary research into military organisational culture and the relative influence of endogenous and exogenous factors on innovation are enormous contributions in their own right. The obligation to produce theoretically sound scholarship informed by nuanced empirical evidence not only protects academic integrity but also increases the likelihood of appropriate practical application. Further, the addition of more overtly critical research strands would greatly enhance the likelihood of military innovation scholarship entering the wider consciousness of the disciplines from which it draws. In particular, there is potential to become more influential within International

⁷⁶ Steven C. Roach, 'Critical Theory', in Dunne, Kurki and Smith (eds.), *International Relations Theories: Discipline and Diversity*, 183.

Relations, where many of the theory challenges facing military innovation scholarship are being played out on a grander scale, and Management, where greater engagement with studies of the organisational dynamics of militaries would diversify the range of relevant public sector case studies and help offset an otherwise heavily business-orientated research base. In both cases, military innovation scholarship has a lot to give and even more to gain from greater theoretical ambition.

Finally, the argument that military innovation studies is not really a field in its own right may have some intellectual merit but as no other disciplines are in any great hurry to stand up and take on responsibility for such a demonstrably important policy-relevant research area, then debate is, quite literally, academic. A healthy disrespect for the sanctity of the name and some creative licence with the field's boundaries would be good things, encouraging an even broader research agenda and a more equal dialogue with related disciplines, but, without the unifying theme of military innovation, the actual research, regardless of its relevance, would be highly likely to fragment, wither and die. This would be a tragedy because whatever we call it, the study of military innovation and institutional learning is a genuinely multidisciplinary field with obvious applied relevance and immense potential to push theoretical boundaries.

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